DOCUMENT RESUME

ED 212 829 CE 031 312

AUTHOR Guba, Egon G.
TITLE The Paradigm Revolution in Inquiry: Implications for Vocational Research and Development.
INSTITUTION Ohio State Univ., Columbus. National Center for Research in Vocational Education.
REPORT NO NCRVE-OP-79
PUB DATE [81]
NOTE 29p.; Paper presented at National Center for Research in Vocational Education Staff Development Seminar (Columbus, OH, 1981).
AVAILABLE FROM National Center Publications, The Ohio State University, 1960 Kenny Rd., Columbus, OH 43210 (OC 79, $2.80).

EDRS PRICE MF01/PC02 Plus Postage.
DESCRIPTORS Behavioral Sciences; Data Analysis; Data Collection; *Educational Research; Models; Position Papers; *Research Design; *Research Methodology; *Research Needs; Research Problems; Scientific Research; *Vocational Education
IDENTIFIERS Naturalistic Research

ABSTRACT

While the rationalistic approach traditionally employed in research and development efforts in the social sciences may be the best method of inquiry to use in the physical sciences, social scientists, and, more particularly, vocational education researchers, would do better to adopt a naturalistic method of research. The naturalist approach to inquiry is better suited for educational research because it recognizes the multiple nature of reality, the necessary interaction between researcher and respondent, and the impossibility of making generalizations among similar situational contexts. Despite the fact that practitioners of the naturalistic approach do not emphasize rigor over relevance as do practitioners of the rationalistic mode of research, their research is not necessarily sloppy. On the contrary, naturalists have their own way of dealing with the issues of credibility, transferability, dependability, and confirmability of data. Adoption of the naturalistic approach would have enormous implications for research and development in the area of vocational education. Among these implications are the following: the notion of the possibility of generalizable research would be abandoned, grounding would be required in every inquiry, human beings would become preferred instruments, and reports would assume the form of case studies or other qualitative portrayals. (MN)

****
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
****

Occasional Paper No. 79

THE PARADIGM REVOLUTION IN INQUIRY: IMPLICATIONS FOR VOCATIONAL RESEARCH AND DEVELOPMENT

Egon G. Guba
Professor of Education
Indiana University

U.S. DEPARTMENT OF EDUCATION
NATIONAL INSTITUTE OF EDUCATION
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

✓ This document has been reproduced as received from the person or organization originating it.

☐ Minor changes have been made to improve reproduction quality.

• Points of view or opinions stated in this document do not necessarily represent official NIE position or policy.

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY

J. Magisos

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)."

The National Center for Research in Vocational Education
The Ohio State University
1960 Kenny Road
Columbus, Ohio 43210

THE NATIONAL CENTER MISSION STATEMENT

The National Center for Research in Vocational Education's mission is to increase the ability of diverse agencies, institutions, and organizations to solve educational problems relating to individual career planning, preparation, and progression. The National Center fulfills its mission by:

- Generating knowledge through research
- Developing educational programs and products
- Evaluating individual program needs and outcomes
- Providing information for national planning and policy
- Installing educational programs and products
- Operating information systems and services
- Conducting leadership development and training programs

For further information contact:

Program Information Office
The National Center for Research in Vocational Education
The Ohio State University
1960 Kenny Road
Columbus, Ohio 43210

Telephone: (614) 486-3655 or (800) 848-4815
Cable: CTVOCEDOSU/Columbus, Ohio

FOREWORD

In a continuing effort to examine major issues that affect vocational education research and development, the National Center for Research in Vocational Education presents distinguished speakers in its National Seminar Series. To share the comments of these experts with you, we prepare occasional papers from transcripts of their speeches.

In this paper, we are proud to share the comments of Dr. Egon Guba on the use of naturalistic inquiry methods to produce research that can result in a better understanding of how humans behave. Dr. Guba, who has his Ph.D. in quantative inquiry from the University of Chicago, has been studying naturalistic inquiry for a number of years. His comments challenge traditional methods of research and open new thoughts of scientific exploration.

Dr. Guba has been an instructor of mathematics and physics and a visiting scholar at three universities and two regional laboratories, and a faculty member at four universities. He is currently a Professor of Education in the College of Education at Indiana University. His publication topics include teacher effectiveness, educational administration, change processes, evaluation, and most recently, naturalistic inquiry. He is a member of the American Education Research Association, American Psychological Association, Phi Delta Kappa, and the National Council for Measurement in Education.

On behalf of the National Center for Research in Vocational Education and The Ohio State University, we present Dr. Egon Guba's comments on "The Paradigm Revolution in Inquiry: Implications for Vocational Education Research and Development."

Robert E. Taylor
Executive Director
The National Center for Research
in Vocational Education

THE PARADIGM REVOLUTION IN INQUIRY: IMPLICATIONS FOR VOCATIONAL RESEARCH AND DEVELOPMENT

In his classic work, *The Structure of Scientific Revolutions,* Thomas Kuhn (1970) has described how a discipline undergoes a shift from one paradigm or pattern for discovering "truth" to another. Such was the case of the shift from Newtonian to relativistic (quantum) physics. Kuhn believes that the institutionalized paradigm, the so-called "normal science," is challenged by the appearance of problems or anomalies with which the old model cannot deal or which it cannot explain. When a sufficient number of such problems or anomalies have arisen, the discipline is confronted by a crisis. Such developments stimulate the emergence of a new, competing paradigm. A struggle ensues between the adherents of old and new paradigms, and, if the new paradigm proves equal to the task, it gradually replaces the old.

Such a struggle is now occurring in the social sciences: between the established "rationalistic" paradigm and the emergent "naturalistic" paradigm. Whether or not the naturalistic paradigm will successfully deal with the problems and anomalies with which the rationalistic paradigm has not been able to cope remains to be seen. There is no doubt that the confrontation will be lively, and that the outcome will have enormous implications for the conduct and meaning of research and development in all fields, including vocational education.

Today, my purpose is to introduce you to this new paradigm and to describe in global fashion what I believe its implications are for research and development. I will begin by briefly describing some of the difficulties with the conventional paradigm that have sent R&D workers searching for new and more satisfying approaches. I will distinguish the new paradigm from the conventional in terms of certain basic differences in underlying assumptions, and will also describe certain "postural" differences between them that have strongly influenced the day-to-day conduct of inquiry. I will also deal briefly with the question of the authenticity of inquiry conducted within the new paradigm, since its critics have elected to censure it chiefly on methodological grounds, charging that it is unbounded, unfocused, and not trustworthy (i.e., internally and externally invalid, unreliable, and nonobjective). I will close with a discussion of the implications of the new paradigm for vocational R&D and for educational R&D in general.

Challenges to the Rationalistic Paradigm

Let me turn to a brief consideration of the reasons that the conventional—what I will call rationalistic[1]—paradigm has begun to be seriously questioned.

First, I would like to point out that while aggregatibility of findings is one of the great claims of pure science (that is, the disciplined nature of science makes it possible for each scholar to build upon what has gone before), no such aggregatibility is evident in the social sciences. Educational research is typically dated from the work of Joseph Mayer Rice published in 1894; thus, we have had more than eighty years of inquiry and investigation. But where are the great principles that have

emerged from all that work? While physicists might trace the evolution of quantum mechanics back through Newton, Copernicus, and even to the pre-Christian writings of Ptolemy, where is the counterpart for educational research? Our work is not aggregatible, and I further suggest that for any given proposition about education, it is possible to find about as many research works (at whatever level of rigor you specify) on one side of the issue as on the other. Practitioners know you can prove almost anything you want to from the research literature. Even worse, findings taken to be true ten or twenty years ago are surely *not* true today.

Second, let me suggest that no matter how useful rationalistic research principles are in the laboratory, they cannot be applied in the real world of social science. The basic conditions that must be satisfied to conform to rationalistic methodology cannot be matched in social or behavioral inquiry. If we consider, for example, the conditions that Campbell and Stanley (who are considered by some to have written the "bible" of research methodology) describe as being necessary for "true" designs, we see at once that we cannot establish needed controls, randomly select and assign subjects to treatment conditions, or blot out all of the possibly confounding factors that may impinge on our inquiry. Of course, some researchers are inclined to dismiss this fact by arguing that nonconformity to design principles arises only because of a false morality or because of political pressures. If we really understood that science was the *only* means to truth, we would not, for example, hesitate to expose our children to possibly inimical conditions of research. Did not half the children in the nationwide test of the Salk polio vaccine receive distilled water rather than the lifesaving serum?

What these researchers fail to recognize, of course, is that truculence or ignorance are not the only factors that produce nonconforming conditions. It is the way of the real world. Human, political, social, and cultural factors *always* influence inquiry. A paradigm that requires unrealistic manipulation of these factors can have but little utility.

Third, I will argue that the traditional research paradigm has an ungrounded quality (Glaser and Strauss 1967). To many researchers, inquiry is a game in which they pit their intellect against nature. The object of the game is to devise a theory or explanation to account for observable phenomena, logically derive hypotheses or questions from that theory, and then test those hypotheses or answer those questions under carefully controlled conditions. The reward, if you guess correctly, is having your findings published in the reputable journals of your discipline.

But it is largely a matter of serendipity whether the theory one starts with has any isomorphism with the phenomena under study. It is no wonder that many people find research to be unreal and meaningless; this approach is largely responsible for the bad reputation that "mere" theory has earned. Inquiry grounded in observation, in a one-on-one wrestling with the real world, is rare to science. Observation is seldom used to generate theory, but only to test it. Thus, it is not surprising that the disjunctions between theory and fact are massive.

Fourth, I suggest that the rationalistic paradigm fails because it depends essentially on propositional knowledge and rules out tacit knowledge. The terms "propositional" and "tacit" (Polany 1958) differentiate between knowledge that can be put into a language form from knowledge that one intuitively knows or understands. People know more than they can say, but the rationalistic paradigm requires that whatever is to be studied must be cast into the form of a hypothesis or question *before* the inquiry begins. Those making the inquiry are not permitted to immerse themselves in a situation, using themselves as tacit instruments to discover what may or may not be *worth* pursuing in that situation. As a result, all our instincts, our intuitive insights, the "vibes" we pick up, cannot become proper objects for inquiry. They must be ruled out as subjective in formation. Much of the valuable knowledge in the social or behavioral areas is eliminated from further study by this arbitrary dictum.[2]

Fifth, it is extraordinary that the major value claim of science is that scientific inquiry is *value free*. The fact that this is not true, even in the physical sciences, is easily demonstrable. One's choice of problem, of method, of analytical tool, and of interpretative theory all are value mediated. Physicists know, for example, that whether light is "proved" to be wavelike or corpuscular in nature depends solely on which method one chooses; for example, Young's slit-diffusion experiment, first carried out in 1903, or Einstein's work on the photoelectric effect early in this century (Zukav 1979). But if one were to stop at this level, one would miss the most important consequence of the value-free assertion: that the real value claims of our inquiring culture are assumed to be objective and rationally justified (valid). We do not stop to consider the possibility that there may exist alternative value claims that should be entertained. In essence, we are objective because we say we are. Such a posture can survive in dealing with physical phenomena, but when it is also applied to social/behavioral phenomena, which are heavily value mediated, serious problems that the conventional paradigm cannot handle emerge.[3]

Sixth, I point to the overreliance on quantative methods in conventional inquiry. I suppose that science has moved in this direction because of its seeming precision, rigor, and objectivity. I recall that several years ago, when I was writing a monograph on naturalistic methods as a visiting scholar at UCLA, one of my temporary colleagues earnestly requested me to urge whenever and wherever I could that the naturalistic methods I was describing not be used by anyone who had not served a long apprenticeship in quantitative approaches. He felt that only a person with such a background could know the true meaning of rigor. But a quick conversion of qualitative phenomena to quantitative indices is a delusion if one is not careful about maintaining an isomorphism between the phenomena being studied and the numbers being used to describe them. Quantitatively oriented conventional inquirers are prone to cast the phenomena of their disciplines into numerical form, manipulate the numbers, and solemnly assert that the results of those numerical manipulations have their exact counterparts in nature. If mathematics show it to be so, who can doubt that it will be found to be so in the real world? But without a thorough grounding in real world phenomena, and when the hypotheses to be tested or the questions to be answered spring largely from conceptual theories devised *before* contacts with reality occur, is it any wonder that this glibly expected isomorphism is often not found to exist?

Seventh, the rationalistic paradigm is burdened by an overemphasis on rigor at the expense of relevance. The requirements of the scientific paradigm (control, comparability, and so on) are so assiduously pursued that virtually no attention is given to whether the findings will have any wider meaning. Urie Bronfenbrenner (1976) has suggested that because of this heavy emphasis on "elegantly designed" experiments, "contemporary developmental psychology is the science of the strange behavior of children in strange situations with strange adults for the briefest possible periods of time." In other words, the preoccupation of scientists with rigor has caused them to carry out studies that can be generalized only to other laboratories. While exhibiting great concern over the conditions for internal validity, they have ignored external validity by default. It is no wonder that persons who are most interested in what happens in the best of all possible worlds (e.g., the laboratory) rather than the worst of all possible worlds (e.g., the real world) find their results uninteresting.

Finally, I suggest that the major indictment one can make of research findings that stem from the rationalistic paradigm is that they have no impact. From a historical point of view, I find it interesting that when Joseph Mayer Rice published his work, *The Futility of the Spelling Grind*, in 1894, he was chagrined that no one paid attention to it. That same tendency—to ignore research findings—has become so evident today that it is one of the first observations that anyone who studies the American educational research enterprise is likely to make. The failure to use evaluation findings is virtually a national scandal. The U.S. Congress is impatient to know why the many studies carried out and many entities developed with federal funds have not produced a "bigger bang for the buck."

It would be simplistic to ascribe all these failings merely to an inadequate paradigm. I am sure that there are other contributing factors for each of the conditions I have described. But I am equally sure that certain basic inadequacies in the inquiry paradigm itself can be faulted. It is my strong impression that both practitioners and researchers in education have intuitively come to doubt the power of the rationalistic approach to provide answers to the questions that are now before us. They are looking for alternatives. Naturalistic inquiry is one such alternative.

The Nature of the Naturalistic Paradigm

When I used the term "paradigm" I am describing a means for arriving at "truth."[4] There are many such paradigms with which we are all familiar, such as the following:

- The logical paradigm, followed in fields such as mathematics, accounting, and philosophy, holds that truth is demonstrable in terms of a priori axioms or assumptions (as in high school geometry).
- The judgmental paradigm, followed in fields such as wine tasting, Olympic contest judging, peer review of proposals, or accreditation visits to campuses, holds that truth is recognizable by knowledgeable experts.
- The *modus operandi* paradigm, followed in fields such as medical diagnosis, forensic pathology, or television troubleshooting, holds that truth can be obtained with some knowledge of the "characteristic causal chains" of phenomena.
- The adversarial paradigm, followed in fields such as criminal law, congressional hearings, or investigative journalism, holds that truth is "emergent" on balance.

The paradigms that have characterized what might be called *disciplined* inquiry, however, have been the incumbent *rationalistic* (or scientific) paradigm and the contending *naturalistic* paradigm. The former, most often found in fields such as physics, chemistry, biology, and the other so-called "sciences," holds that truth is demonstrable or confirmable. The latter, typically found in fields such as ethnography, history, and political science, holds that truth is ineluctable; that is, it is inescapable when experienced over a sufficient length of time.

Which of the paradigms is "right"? There is, of course, no answer to that question; if there were, the field of epistemology would disappear, and there would be no further room for debate. Unfortunately, there is no meta-paradigm or meta-criterion that might help us make an infallible decision. There is no point to debating relative rightness, for the question remains forever indeterminate. How then can one make a choice?

These various paradigms, including the two of greatest interest to us here, all rest on certain *assumptions*. When some inquiry is to be conducted, and the question arises as to which paradigm is preferable for conducting it, the answer should be determined on the basis of *which set of assumptions is best met by the inquiry in question*. Just as one would not use a statistic without asking whether its assumptions were met by the data to be analyzed, so one cannot choose a paradigm without asking whether *its* assumptions are met by the phenomena to be investigated.

Using such logic, we must now ask two questions. First, what are the basic assumptions of these two paradigms? Second, which set is more useful for the phenomena that social/behavioral researchers investigate?

Let me begin by delineating the assumptions as I understand them. I ask you again to hold in abeyance your "natural" tendency to choose one or another set as more self-evident. You may recall that when you studied high school geometry, you were instructed that geometric proofs consist of demonstrating that derived theorems were logical consequences of a small set of axioms. When you asked what axioms were, and where they came from, you were told that they were self-evident truths. But of course that is not so, as subsequent developments in geometry have demonstrated. There are many different sets of axioms that can be adopted: non-Euclidean assumptions yield a variety of exotic geometries. And while *these* axiom systems often do not appear plausible, let alone self-evident, it turns out that all of them have validity for certain kinds of analyses. For example, if you are working with interstellar spaces, you are better off with Lobachevskian geometry than with Euclidean, even though the basic axioms of Lobachevsky are mind-boggling (e.g., given a point and a straight line, it is possible to construct many lines through the point—*all* of which are parallel to the line). You must suspend your disbelief for the moment; we shall later return to the question of which set of axioms is more *tenable* for social/behavioral inquiry, however unbelievable they may be at first sight.

ASSUMPTION 1: The nature of reality

Rationalistic axiom: There is a singular reality, which is fragmentable into bits and pieces (variables); all of which can be independently studied, and onto which inquiry can ultimately converge as more and more studies are done.

Naturalistic axiom: Reality is multiple (note the word *multiple:* the naturalist is not claiming many perspectives on some single reality, but *multiple realities*), which must be studied in holistic forms (or else they are destroyed), and from which inquiry will ultimately diverge as more and more studies are done.

ASSUMPTION 2: Subject–object (respondent) interaction

Rationalistic axiom: It is possible for the inquirer to maintain a discrete (also discreet) distance from the object or inquiry, so that neither influences the other.

Naturalistic axiom: The inquirer and the respondent necessarily interact; each influences the other to some degree.

ASSUMPTION 3: The nature of truth statements

Rationalistic axiom: The aim of inquiry is to devise nomothetic laws or generalizations (enduring truth statements that are context-free); generalizations are discovered by focusing on the similarities among different things.

Naturalistic axiom: The aim of inquiry is to devise idiographic working hypotheses that "fit" a particular situational context; generalizations are not possible, although there may be transferability between two similar contexts. Differences among things are as interesting as similarities.

Which of these sets of assumptions is better met by the social/behavioral phenomena which constitute our unique domain of inquiry? I suggest that the naturalistic paradigm is a hands-down winner. Consider the following.

1. *On reality.* For the physicist or chemist, the singular view of reality embraced by the rationalistic paradigm seems indeed appropriate. It would be absurd to deny the self-evident reality of physical objects. The basic terms of physics—force, mass, time, velocity, and the like—demonstrate

the utility of dividing the world into variables and studying their relationships. Successive experiments can converge onto some physical constant or basic principle. But for the social/behavioral inquirer, there is no physical reality. The "realities" being dealt with here are *constructed* realities; that is, realities that are devised in the minds of persons to extend meaning to events and to render them interpretable and understandable. Everyone has an individual construction to account for his or her world. These constructions identify friends and enemies, what job demands are, whether the person is healthy or ill, and so on. Political interpreters (reporters) make a living by constructing realities about the events that take place at high political levels. Madison Avenue writers create realities for us with respect to cosmetics, autos, or soft drinks. We social scientists construct realities that we call theories. *But these constructions are not isomorphic to any real situations,* as might be claimed for physical science constructs and theories. They exist *only* in people's minds. Each construction is a complete whole; what would be the value of sundering a mental image? There also is no end to these constructions; inquiry cannot converge on them for there is, ultimately, nothing to lock on to. Indeed, the situation is quite the opposite, for as these multiple constructions are investigated it is apparent that they lead only to more constructions.

2. *On interaction (dualism).* That there is no interaction between investigator and investigated object is a reasonable assumption in virtually all areas of physical science. Chemicals react in test tubes as they do in nature; they are insulated from the presence of the investigator. (But note that in the area of particle physics, the Heisenberg Uncertainty Principle challenges the assertion of independence even with respect to physical objects.) But it certainly cannot be claimed that there is no interaction between investigator and object when that object (the naturalist would prefer the term *respondent*) is another human being. Social/behavioral science has long been aware of the possibility of *reactivity,* a particular response to the investigator. But it also is the case that the investigator can react to the respondent, and that both *interact* with each other. Within the rationalistic paradigm, such interaction is assiduously avoided; indeed, it is part of the mystique of scientific methodology that it is supposed to *guarantee* that the results of an inquiry will be untainted by such subjectivity. But denial of an effect will not change its impact; interaction is there (as anyone who has ever conducted research on humans can attest), and it does make a difference. The naturalist, far from being overwhelmed by this circumstance, welcomes it because interaction makes it possible for the investigator to *learn* during the conduct of study, to adapt his or her understanding and methods, and ultimately to redesign the study as it progresses. For the naturalist, the gain in understanding and flexibility is a more than adequate trade-off for the objectivity which is (putatively) lost. Such objectivity is never possessed by the rationalist.

3. *On generalization.* The concept of generalization is an inherently appealing one. Who would not, given a preference, opt for universal laws and principles that would hold at all times and in all places? And surely, for the physical scientist, such an ambition is not beyond achievement. That force should equal mass times acceleration (F=MA) does not surprise us, whether in the eighteenth or twentieth centuries, whether on Earth or on Mars, and whether the accelerated object is a ball, an automobile, or a psychologist. Indeed, if anyone doubted this fact, the investigation that led to the conclusion could easily be repeated. But what of human behavior? The essence of a generalization is that it is time and context *free,* and who can name even one instance of human behavior that is not heavily mediated by the situation in which it occurs? Two professors discussing a point will interact very differently if they are part of a symposium at the annual meeting of a professional association than if they are at a local faculty meeting. They will again interact very differently if they have run into one another in the restroom. The naturalist would not deny that there can be transferability from situation to situation, but the degree of transferability can be judged only if one knows a great deal about both transferring and receiving contexts so that the extent of "fittingness" can be determined. If the essence of generalizability is to remove all contextual traces, the essence of transferability is to add as many contextual details as possible. The former is a nomothetic move, typical of rationalism; the latter an idiographic move typical of naturalism.

From this analysis I conclude (and hope to have persuaded you) that the naturalistic paradigm is the paradigm of choice whenever one deals with social/behavioral phenomena. That conclusion is not intended to demean the utility of the rationalistic (or any other) paradigm for other purposes. Given the history of the hard and life sciences, for example, one would be a fool not to recognize the power of the rationalistic paradigm for those areas. But in the world of the mind, the personality, the social group, the culture, one is equally foolish not to recognize the power of the naturalistic paradigm.

Some Postural Differences

While the rationalistic and naturalistic paradigms are differentiated most cleanly by the differences in their fundamental axioms, it is also the case that they are differentiated by certain methodological postures. Practitioners working from these two paradigms are not *compelled* to take these postures, but seem to have embraced them for historical reasons. (We do what we do because that is the way our mentors did it, just as we may believe what we believe because that is what our parents believed.)

Several of these postures have already been alluded to in the opening paragraphs of these remarks; these postures, it was charged, are responsible for some of the crises that confront the inquiry community today. Rationalists have insisted on emphasizing rigor over relevance, a priori over grounded theory, quantitative over qualitative methods, and propositional over tacit knowledge. But it should quickly be noted that naturalists have begun to show signs of an equally unsettling orthodoxy: relevance over rigor, grounded over a priori theory, qualitative over quantitative methods, and tacit knowledge as co-equal with propositional. But we cannot allow the situation to degenerate to a mere contest of wills, of winner take all, of right once-and-for-all. If the paradigms should be examined for degree of fit between assumptions and phenomena, so should the postures be examined for balance. There is no intrinsic reason why an investigator, using *either* paradigm, cannot or should not be concerned with both rigor and relevance, cannot use both quantitative and qualitative methods, cannot invoke a priori theory so long as there exists grounding at some point in its development, and cannot admit both tacit and propositional knowledge so long as some safeguards (see below) can be established to preclude *mere* intuition and guesswork.

There are other postures that might be mentioned as well, including—

- *reductionist versus expansionist stance* (convergent versus divergent inquiry; focusing the inquiry on a small number of predetermined variables versus focusing on holistic emergent patterns);
- *verification versus discovery purpose* (verifying existing hypotheses rather than discovering new ones);
- *"objective" versus "subjective" instrumentation* (using paper-and-pencil devices or physical devices, e.g., the polygraph, as instruments versus using the self, or another human being, as an instrument);
- *preordinate versus emergent (or cascading or rolling) design* (specifying each step in the inquiry ahead of time versus making each step contingent on all preceding steps);
- *intervention versus selection style* (manipulating the situation to produce the circumstances to be tested versus sorting through naturally occurring situations to find examples in which Nature has already arranged the experiment);
- *laboratory versus natural setting* (carrying out studies *in vitro* versus carrying them out *in situ*);

- *stable versus variable "treatment"* (insisting on holding all treatments invariant during the course of the study versus allowing them to change as the situation may require);
- *discrete versus holistic analytic units* (doing analysis in terms of discrete data chunks —variables—versus holistic pattern analysis); and
- *closed versus open inquiry* (emphasis on control versus willingness to "invite interference"—inquiry in the best of all possible worlds versus inquiry in the worst of all possible worlds).

For each of these postures, an appropriate balance seems to be the ideal toward which inquirers should strive. Of course, the rationalistic posture has been overemphasized for so long that a more aggressive move in the direction of naturalist preferences may be appropriate for the time being. But a new orthodoxy must be avoided at all costs.

Is Naturalistic Inquiry Necessarily Sloppy Research?

While more and more persons seem to be willing to concede the occasional utility of naturalistic inquiry, that willingness is frequently hedged by an underlying conviction that such inquiry is inherently flawed: it simply cannot be authentic to the same degree and in the same way as is rationalistic inquiry. Naturalistic inquiry is, in the parlance of the rationalist, "soft," "mushy," "spongy." Is that charge justified?

There seem to be three major discussion arenas of putative "softness."

1. *Bounding an inquiry.* It is claimed that inquiry which is expansionist, emergent, focused on discovery, permissive of variable treatments, open to invited interference, and so on, is necessarily unbounded. How can the naturalist decide what is or is not relevant to an inquiry under those circumstances? The naturalist may claim to be open-minded, but may just be empty-headed.

2. *Focusing an inquiry.* If all options are open at the beginning of a naturalistic inquiry, how can the inquirer sort the information collected into useful patterns? How, in the parlance of the trade, can the data be unitized and categorized?

3. *Establishing the trustworthiness of an inquiry.* How can the naturalistic inquirer demonstrate that the information gathered meets the traditional criteria of trustworthiness—internal and external validity, reliability, and objectivity? Does not the choice of paradigm effectively preclude being able to meet these criteria satisfactorily?

Let us consider each of these charges in turn.

Bounding an inquiry. The task of bounding an inquiry is not essentially different for the naturalistic inquirer than it is for the rationalist. Both begin with an inquiry *problem,* and the nature of that problem is the key element in setting boundaries. I have discussed the nature of problems and the ways that problem statements can be used to generate inquiry boundaries in other contexts (see Guba 1978, and Guba and Lincoln 1981). If there is a difference in the bounding situation for the rationalist and the naturalist, it is simply that the rationalist insists on establishing boundaries *before* the inquiry is undertaken, while the naturalist is content to allow those boundaries to emerge *during* the inquiry. It is possible for rationalists to establish a priori boundaries, because they tend to work from a priori theory with specific questions or hypotheses posited entirely at the propositional level, and with preordinate designs that will be held constant throughout the inquiry. The

rationalists then turn this capability into a virtue, indeed, a criterion, by claiming that because they do everything in advance, they are free from the influence of investigator bias or the tendency to capitalize upon accidental trends in the data. Thus they are all the more objective. Naturalists are, however, no less rule guided or systematic; if they change rules in mid-study, they return to earlier data and reanalyze them by the newly emergent rules, so that all data have received the same analytic and interpretative treatment in the end. Moreover, by keeping their boundaries open, naturalists gain the flexibility to adjust to new insights, to learn continuously (they do not have to have it all down ahead of time), and to *redefine* the problem as is necessary. But an important point must be made. At every stage of the game, naturalists, like rationalists, are guided by *some* form of problem statement; decisions about what to include or exclude are as easy (or difficult) for them as it is easy (or difficult) for the rationalists to explicate hypotheses or raise questions.

Focusing an inquiry. Rationalists, by virtue of having a priori hypotheses on questions, are able to specify exactly what kinds of data they will collect, using what instruments, employing what analytic tools, and reporting in what form. (They are often able to make up "dummy tables" ahead of time, so certain are they of what the inquiry will produce.) Naturalists, on the other hand, begin data collection on a much more intuitive basis, and end with accumulations of observational notes, interview protocols, collected documents, and so on. All of these materials are in very primitive (the rationalist would say "raw") form. How can naturalists process all these data, drawn from such disparate and possibly nonaggregatible sources, into something resembling reasonable categories? How can they decide what particular data items go where in this set of categories?

There can be no doubt that the unitizing and categorizing task is more difficult when one confronts masses of raw data already collected than when deciding that there are only three variables that will be measured, that they will be measured with a test, a questionnaire, and a physical measurement, and that they will then be processed using the appropriate program from SPSS. But in principle, the tasks confronting the rationalist and the naturalist are identical; they must ultimately decide what can be drawn out from the variety of possible data that will best speak to the problem being investigated. Again, I find the crucial difference to be *timing.* A metaphor that has been useful to me in considering this problem is that of preparing a test for any of the several courses I teach. I can do all my work *ahead* of time by developing a highly valid multiple-choice test with items that are all pretested, and for which I know the difficulty and discrimination levels. The development process will require many hours and may span weeks of time. But once the test is developed and administered, it is literally a matter of minutes to have it scored by computer, have a grade sheet printed out, and post the results. On the other hand, I may choose to do all my work *after* giving the test. I can devise a good set of essay questions in about one-half hour, and my secretary can easily type and duplicate the single page of items. But, when I receive back ten pages of hand-scrawled responses from each of my twenty-five students, I am confronted with a formidable task indeed. Many hours, possibly weeks of time, may be required to read and judge all these responses. Similarly, in focusing an inquiry, I can specify all of the elements ahead of time (a process that may require weeks of thought), so that when I receive the data, I simply code and punch them and, in a matter of minutes, have my computerized analyses ready. Or, I can collect data in a more open-ended way, and then spend my weeks putting them into logical categories.

Yet the authenticity of my data does not depend in any way on *which* of these choices I make. I do reap one important advantage by operating in the naturalistic mode, however. Since I can begin data analyses with my first data source (my first interview, my first observation, my first document), I can maintain an open or emergent design, with subsequent data collection and analysis profiting from everything I have learned to that point (as against being informed only by what I was able to guess ahead of time).

Establishing the trustworthiness of an inquiry. Trustworthiness strikes me as an especially important issue precisely because it is the area in which naturalistic inquiry is most frequently and most vigorously attacked. In another context (Guba 1981) I have dealt with this problem with some thoroughness, but I will essay a brief treatment of this complex topic here.

Criteria for trustworthiness and procedures for meeting these criteria have been well evolved within the rationalistic paradigm. Four questions may be used to identify factors that provide "trustworthiness" assurances.

1. How can one establish confidence in the "truth" of the findings of a particular inquiry—given the type of subjects involved and the context within which the inquiry was carried out? This is, in rationalistic terms, the question of *internal validity;* the rationalist's solution is to control all possible confounding variables or relegate them to the status of randomized variables. Random selection and assignment of subjects to treatments, plus overt control of whatever confounding variables seem most important (e.g., intelligence in a learning study), provide an *unassailable* defense against the charge that a study is not internally valid.

2. How can one determine the degree to which the findings of a particular inquiry may have applicability in other contexts or with other subjects (respondents)? This is the question of *external validity;* the rationalist's solution is to choose a sample group that is representative of the population for which generalization is desired. Here, probability sampling is the unassailable defense.

3. How can one determine that the same results would be found *consistently* if the inquiry were repeated with the same (or similar) subjects in the same (or similar) context? This is the question of *reliability;* the rationalist's solution is to demand reliability indices that indicate the extent to which replicability can be attained by the instruments, the judges, the coding, and so on. Reliability coefficients that are in excess of commonly agreed upon levels represent an unassailable defense.

4. How can one establish the degree to which the findings of an inquiry are solely a function of the subjects and the conditions of the inquiry—and not of the biases, motivations, interests, perspectives, and so forth, of the inquirer? This is the question of *objectivity;* the rationalist's solution is to devise foolproof methodology that effectively insulates the investigator from the objects of inquiry, and thereby guarantees that his or her own predispositions cannot influence the outcome. Demonstration of faithful adherence to this methodology (scientific method) provides an unassailable defense.

Naturalists have their own ways of dealing with these four generic questions. The same questions are as appropriate for the naturalist as for the rationalist, but because of the epistemological differences in their approaches, both the *interpretation* of these questions and the responses made to them differ. We may again consider each in turn.

1: *Credibility.* Rationalists, making the assumption of a single and realizable reality, consider the test of internal validity to be the degree of isomorphism between their data and that reality—is there a one-to-one correspondence?[5] The naturalist assumes multiple realities that are constructed, i.e., that have no real counterpart. Yet the idea of isomorphism can still be utilized. The multiple realities, the constructions, exist in the minds of people; hence, the test of isomorphism is to determine, from the people, whether they find the data and the interpretations made from them credible.[6] *Credibility,* determined by actually checking with the persons who were the data sources (a process often called "member checking"), is thus the cornerstone of the naturalist's approach to internal

validity. To build the probability that a study will be judged credible, naturalists engage in certain activities or pose certain safeguards. They use prolonged engagement at a site to be certain they have identified what is truly salient in a situation. They persistently observe factors that emerge as crucial. They debrief themselves with peers who are charged to ask difficult questions and to help them maintain their sense of identity lest they "go native." They triangulate their data from multiple sources, using multiple methods and perspectives, and, if possible, multiple investigators. They collect and store materials (called referential adequacy materials) that can later be used as points of reference if some interpretation is in doubt—videotapes of classrooms, documentary classroom products, and so on. Finally, they engage in the member-checking process on a day-to-day basis (for example, checking yesterday's interview data with today's respondents) so that they have immediately useful credibility feedback.

2. *Transferability.* For the rationalist, the aim of establishing external validity is to guarantee the generalizability of whatever the study generates. The naturalist is unconvinced of the possibility of generalization, however, except in the limited sense of transferability of findings from one context to another if there is substantial similarity between the two. *Transferability* is thus the key term for the naturalist. Two matters are of importance here. First, the naturalist is hardly ever concerned with sampling as the basis for transferability; whatever samples he or she draws upon are likely to be theoretical (Glaser and Strauss 1967) or purposive (Guba and Lincoln 1981) in order to provide the *widest possible band of information* instead of *typical* information. Second, the naturalist will collect "thick" descriptive information about the context so that, if the question of transferability to a second context comes up, enough will be known about the studied context to make possible a judgment about the degree of similarity between the two.

3. *Dependability.* Within the rationalistic paradigm, reliability is important because it is a prerequisite for validity; methods must produce stable results if those results are to be meaningful (recall the old measurement theorem that the validity of a test cannot exceed the square root of its reliability). Although naturalists are also concerned with stability for the same reasons, the concept is trickier in this realm. Since the rationalists assume a single reality upon which inquiry converges, they can treat all instrumental shifts as error. Naturalists, in assuming multiple realities and in using humans as instruments, must entertain the possibility that some portion of an apparent instability can be accounted for by shifts between realities or in the thrust of the instrumentation. Factors in such a situation change not only because of errors (mistakes or fatigue, for example), but also because of evolving insights. Thus, for the naturalist, what is important is not that there is invariance, but that whatever variance does occur must be *trackable,* that is, accountable. It must be assignable to sources: so much for error, so much for reality shifts, so much for increased instrumental proficiency, and so on. For the naturalist, then, the key term is *dependability,* a concept that embraces elements of the stability implied by the rationalistic term *reliability* and of the trackability required by explainable changes. To provide some hedges, the naturalist may use overlapping methods (a form of triangulation; see for example, Webb et al. 1966) or split-teams (see "stepwise replication" in Guba 1978, and Guba and Lincoln 1981), but the most powerful tool is the *dependability audit* (Guba and Lincoln 1981; Guba 1981): a process that parallels the work of the fiscal auditor insofar as such auditors certify that the accounting procedures used are within normally accepted practice for the accounting profession (i.e., that there has been no "creative accounting"). In permitting such an audit, the naturalist will maintain an "audit trail," or complete documentation for every process step. An essential part of this audit trail is the daily journal, in which methodological entries are systematically made.[7]

4. *Confirmability.* Objectivity has been a major preoccupation of rationalistic followers, possibly because the chief value claim of the rationalistic paradigm is that it is value free. But objectivity is a "Holy Grail"; it is absurd to suppose that one's *actions,* which are determined by

a series of human decisions, can somehow be rendered *free* of human predispositions. Methodology inevitably reflects the constructed realities of the investigators; the fact that they have decided to share a common, constructed reality makes no difference. One will inevitably find what one sets out to find, as in my earlier allusion to the wave or corpuscular nature of light. In the social sciences, the cultural and ethnic biases that can be built into so-called "objective" instruments such as IQ tests are well known. Naturalists are especially aware of this problem because they understand the multiple realities (including multiple value systems) that can be encountered, and the roles that inquirers' own dispositions can play when they use themselves as instruments. Following the reasoning of Michael Scriven (1972), the naturalist shifts the burden of objectivity from the investigator to the data, requiring evidence not of the *certifiability* of the investigator, but of the *confirmability* of the data. To build evidence for confirmability, the naturalist engages in data triangulation and practices reflexivity (Reinharz 1979, Spradley 1979, Ruby 1980), leaving an audit trail that enables a *confirmability* audit (Guba and Lincoln 1981; Guba 1981) to be done. Such an audit parallels the work of the fiscal auditor seeking verification for every entry in the fiscal journal (the receipts, vouchers, and so on that attest to the legitimacy of entries), and for the totals that are calculated therefrom (the bottom line or the final interpretations and conclusions).

From this analysis I conclude that it is possible for the naturalist to pose counterpart concepts to every "trustworthiness" criterion that has traditionally been used by rationalists, and to devise procedures that increase the probability that the criteria will be met. But you may recall that in my earlier summary of rationalistic methods, I asserted that evidence of random selection and assignment of subjects, plus control of the chief confounding factors involved in the inquiry, provided an *unassailable* defense against a charge of poor internal validity. I also asserted that evidence of representative sampling from a population provided an *unassailable* defense against a charge of poor external validity, and so on. You may also note that I did *not* claim that the measures taken by naturalists constituted similarly unassailable defenses. Instead, the best naturalists can hope for is to increase the probability that they will have devised a *minimally persuasive* defense. In that sense, the naturalistic theory of trustworthiness is an incomplete one—one cannot muster evidence that will compel another to accept the trustworthiness of the study, but only evidence that will persuade the other of its *relative* trustworthiness. But this situation neither surprises nor dismays naturalists—such indeterminacy is what they *expect* of the "real" world. The naturalists' response to someone who cannot tolerate that degree of ambiguity is simply to say, "Whoever promised you a rose garden?"

Implications

I have already asserted that the decision about which paradigm to use in guiding an inquiry must be made in terms of the degree of fit between the phenomenon to be studied and the assumptions of each paradigm. I am sure that there are some areas of inquiry in vocational education for which the rationalistic paradigm is best suited, and in those cases it should certainly be used. But I am equally sure that the large bulk of the R&D done in this field (and indeed, in all educational fields) would be better served by the naturalistic paradigm. If that paradigm were to be adopted and widely applied, the implications for doing, reporting, and applying R&D would be enormous. I will try to deal with a few of them.

1. *The entire conception of R&D would be forever altered.* Virtually all the vocabulary associated with R&D would disappear or be fundamentally changed. My wife, Yvonna Lincoln, and I have joked from time to time about what we call the "methodism" of educational R&D, intending the term to be parallel to the term "sexism." Just as all of our cultural, often implicit stereotypes about females are caught up in the structure of our language, so are our stereotypes about research. Consider what it would mean to have to do without the term *variable.* Who would

want to use it after giving up the assumption of a single, fragmentable reality? What would it mean to conduct research without terms such as *control, comparison group, representativeness, aggregatibility,* and so on? Other words that now have one meaning would mean something else—*sample* would denote error *plus* trackable "true" variation, and so on. In short, to adopt this paradigm implies a complete restructuring of the way we think about R&D.

2. *The idea that there could be generalizable research that could lead to generalizable developments would be abandoned.* If generalization is not possible, because all social/behavioral phenomena are so intensely context mediated, it is patently not possible to devise research-based developmental products that have universal applicability. Instead, developments capable of broadband retuning and local adaptation would be required so that they could be fitted to whatever factors dominated in the local context.

3. *Developments could not receive a "final" evaluation or be given a guarantee that warrants them for general use.* While a product's *merit* (its intrinsic value as determined, for example, by its modernity, its internal consistency, its integration, and so on), might be established that way (its merit accompanies the product into any context), its *worth* (its extrinsic value as determined by its usefulness) can be established only with respect to actual contexts. In other words, worth must be separately and independently determined in each and every context in which application is proposed (Lincoln and Guba 1980).

4. *Grounding* (Glaser and Strauss 1967) *would be required in every inquiry.* Current inquiry is usually ungrounded in the sense that it is based not upon multiple constructed realities that characterize the respondents' view of the world, but upon the single constructed reality that is the inquirer's guiding theory. As Perrow (1981) has stated so succinctly:

> We social scientists try to eliminate disorder with rational designs. ... Write articles with simple, orderly, elegant, and inclusive theorems or hypotheses or models: this is what the world should look like. Test the model with questionnaires that subtly create for the respondent the world we want to prove exists, what Orne calls "the demand characteristics of research instruments"—demanding the kind of behavior you wish to prove exists. Force a tight coupling between the work and the deed.
>
> We force that tight coupling by our view of what science should be. We assume that rational accounts of even so-called irrational behavior can be constructed and then confirmed by testing hypothese generated from the accounts. But every step of the way contains self-imposed deceptions. The questions we pose to subjects assume the subjects share the world the social scientists have imagined, and in the course of this questioning we elicit responses favorable to that world conception. (p. 6)

5. *Human beings would come into fashion as preferred instruments.* The instrumentation that is common today could not exist when the design is emergent, when the problem or the methodology can shift in midstream, when data are focused, categorized, and analyzed by developing rules, and when hypotheses or questions come into focus only after the inquiry has progressed. To maintain the needed degree of flexibility and adaptability, an instrument is necessary that can *learn,* in short, a human being. I have always found it odd that in scientific inquiry we tend to reverse the order of priority accorded to evidence in legal courtrooms. There the best evidence is eyewitness evidence; evidence that cannot be corroborated by a witness is termed "circumstantial," and virtually no case can be won on circumstantial evidence alone. But we researchers denigrate human testimony, calling it "merely subjective," while exalting circumstantial evidence such as test scores and questionnaire responses. Use of the naturalistic paradigm will surely militate against that traditional posture.

6. *Reports would lose their present technical, quantitative orientation and take the form of case studies or other qualitative portrayals.* When there is no generalization, the statistical table that summarizes everything worth knowing cannot be produced. If the context requires "thick description" to be understood, substantial portions of reports would have to be devoted to such descriptive passages. The aim of reporting would be to improve the understanding of audiences, rather than to impress professional colleagues with technical niceties. The "executive summary" that boils everything down to a few summary principles would have to be abandoned—the reader who is unwilling to invest enough time to become immersed in the data would not be able to understand the report, for there would be no shortcuts.

7. *Policymakers would need to reorient themselves to the use of R&D data.* At the moment, policymakers are demanding—and getting—R&D information in highly compressed form. Given the democratic form of government under which we live, it is not unreasonable for the lawmaker, for example, to look for a single number or a single phrase that best "represents" constituents' needs. And the rationalistic paradigm offers that lawmaker every assurance that he can have what he wants—that it *is* possible to summarize matters in a few numbers, to develop generalizable principles on which the lawmaker can act, and to prove them on the basis of one grand, national, numerical experiment. Information generated under the naturalistic paradigm does not have those qualities, however, and while it may be disappointing or frustrating to policymakers to deal with information on a much broader, nonaggregated, nonsummarized, qualitative differentiated way, better decisions will surely result in the long run.

8. *The R&D worker who shifts to the naturalistic paradigm will take grave risks.* It must be remembered that it is the naturalists who are challenging and the rationalists who are entrenched. Politically, rationalists can seem to offer the public what it wants. Methodologically, naturalism is not nearly so well developed and is open to multiple challenges. Naturalists who apply for funding are likely to confront program officers who neither understand nor appreciate what they are trying to do. (Students have a parallel situation in seeking to persuade dissertation committees of the legitimacy of what they propose to do.) Reports are likely to go unused because readers do not find a "quick-and-dirty" executive summary, or a few numbers or phrases that they can easily incorporate into their thinking. Journal editors and publishers are unlikely to be able to judge the quality of work done in this mode, and may reject papers or monographs for no other reason than that the thick description they contain exceeds their publication's space limitations. Thus, one can predict that the way will not be easy for the inquirer who decides to "go naturalist." Fortunately, the uncomfortable position that is created by using this paradigm has never deterred those persons who have felt a special sense of mission. I trust that such dedication will continue.

9. *Finally, a measure of value-resonance will be restored between the substance of our inquirers and the methods we use to engage in those inquiries.* The claim that the rationalistic paradigm is value free has, by definition, obscured the problem of the relation between substantive and methodological values in inquiry. I have already quoted Perrow (1981) as asserting that our research has certain demand characteristics—that we find our world to be what we structure it to be. If this is the case, it seems imperative to me that when we do make inquiries, we use a paradigm whose assumptions (values) fit those of the field we are inquiring into. Perrow makes the point in relation to the study of administration. He says:

> To put a concept on this point and take my next step in dismantling social sciences, let me observe our world is more "loosely coupled" than our rationalistic theories would ever allow. What loose coupling means is that there is a great deal of redundancy, slack, and waste, and it is when we ignore this that we are successful at accounting for reality, at explaining the world

> in a rational way. . . . In time, we convert all the aspects of loose coupling—the slack, buffers, redundancy, and waste—into a benign overview of a well-functioning system. We convert micro-confusion into macro-order. (p. 6)

To put it another way, if we are to deal with "loose coupling," we cannot study it with a methodology that presupposes tight coupling; there is then no value-resonance, and erroneous conclusions will surely result.

Another example: a graduate student in reading at Indiana University with a minor in methodology is currently writing a paper for me that relates methods of inquiry to theories of reading. Classic theories of reading, she asserts, treat reading skills as elements that are external to students. Yet these are skills that they must acquire. These skills can be separately learned—work recognition, elements of phonics, and so on. If one wishes to study reading under those theories, a rationalistic approach seems quite appropriate: the reading process can be broken into separable bits called skills, each skill can be independently manipulated, the interactions of each skill with student characteristics such as age, sex, IQ, and so on can be determined, and so on. But our reading department is interested in a rather different view of reading. In this view, the process of reading is treated as a holistic process that is internal to the individual. It is not separable into components. If students study reading from *that* theoretical perspective with a rationalistic paradigm, how can they expect anything to result except unintelligible data?

It is my contention that there has been a similar value misfit between most of the phenomena we are interested in studying and the methods that we use to study them. We did not make an unreasonable mistake. After all, the rational methods of science had scored prodigious successes in fields such as physics, chemistry, and biology. The scientific method provided an aura of legitimation for our fledging inquiries that would have been impossible to attain in any other way.

We can perhaps be forgiven if we did not notice, at first, that there was severe value dissonance between the conventional methods of science and the problems confronting the social/behavioral inquirer. But we are now in a position to appreciate that fact, and we no longer need the legitimation that the scientific method once bestowed on its adherents. It is time, in short, to break away; despite the risks, despite the upheavals, and despite the reorientations that will be required. For most of the purposes we seek to achieve, the naturalistic paradigm is *the* method to choose.

QUESTIONS AND ANSWERS

Question: Your articulate and stimulating point of view is useful in giving us an opportunity to reexamine the presuppositions on which the scientific method rests. But your suggestion that we replace the hard-won scientific techniques in the social/behavioral domain with an unproven set of "naturalistic" observational procedures reminiscent of the anecdotal reports and case studies of decades past is unconvincing. Not only are you throwing the baby out with the bathwater, you seem to be locking up the bathroom and leading us back to the outhouse.

Toward the end of your paper you acknowledge that the "rationalistic" approach may be better for investigating some questions, though you prefer the "naturalistic" approach for most purposes. Since the title of your paper mentioned "Implications for Vocational Research," what are some examples of vocational research questions that are best suited for each approach?

You're concerned that I go too far. I know at times, the paper takes a very extreme position that says all human behavioral questions ought to be attacked by naturalistic inquiry. Later on I leave the door open for at least some questions that might be attacked by rationalistic inquiry. In regard to your question on the title of the paper, I chose that particular title with trepidation. I would have preferred to leave off "Implications for Vocational Research and Development" because I do not know enough about the field to understand what the implications are. I don't know what the major research issues or questions are. The assertion I want to make is that the choice of method is up to your assessment of what you believe are the best fit assumptions. I traveled here yesterday and spent the night with some friends in Columbus. They had a friend that they invited over for dinner who got very interested in this same particular problem and was pushing me very hard on it. I tried to give some examples, and it turned out that her background was in the biological sciences. She said, "All of my training leads me to believe that human behavior is ultimately genetically determined. Whether you believe that or not, that's the theory I propose to work from; and if I take that posture, wouldn't it be true, by your own analysis, that I should apply the scientific analysis because genes, after all, do have a tangible reality?" My response to that was to say, of course—if you really thought that you could account for behavior by some series of genetic expressions that could ultimately be linked back to the biological genes in each of the cells. I would suggest that you have a substantive theory whose assumptions are very like the assumptions that are rationally secure. You, therefore, ought to use the rationalistic method. But, if you believe as I do that at best only a small portion of human behavior is genetically determined, and that much of the rest of it is contextually determined or determined through the experience of earlier contexts, including the experiences at mother's knee, and so forth, then I do not believe that the assumptions of the rationalistic paradigm fit nearly as well as the assumptions of the naturalistic. I keep asserting that behavioral phenomena are different from physical phenomena. I know that some portion of vocational education is given over to the development of skills. And, in that sense, there would be an analogy to my example from reading. If you believe that the vocational behavior, if there is such a term, of people has only to do with the acquisition of certain kinds of skills, and if you want to research how those might be most effectively taught, you would probably be well advised to use the rationalistic paradigm. On the other hand, there may be other aspects of vocational behavior, such as career choice, that are largely a matter of reality construction. For example, observation of certain kinds of role models or exploration of one's attitudes and interests with respect to career choice might find the naturalistic paradigm much more congenial. I do not think that there is any way of describing in an internally true way the process by which career choice decisions are made. What I urge you to do is to stop and ask yourself, "How am I conceptualizing this? What method would fit?" Then

try to pick accordingly. I think one of the implications of what I am saying is that two different people attacking the same problem may formulate the problem in sufficiently different terms that one would be led to use of the rationalistic paradigm and the other to the naturalistic paradigm. I have no objection to that as long as they understand the game they are playing. They have to be explicit about the assumptions and make an informed, intelligent choice rather than just slipping into it because that is the way we have always done it.

Question: If I understand your distinction between single and multiple realities, the naturalistic approach denies the universal nature of certain societal processes such as the production and distribution of goods and services or the transmission of culture across generations. If you deny such universal processes, what happens to scientific inquiry? If you do not have cumulative, generalizable knowledge, all you have is careful journalism.

You can define it that way. Science has those characteristics, and those things that you are urging us to do fail in one or another or both of those tests, and therefore, they cannot be science. It is not important what you call it. I do not much care for the label "scientific" anyway, because in our culture science is accepted as truth. It is such an orthodoxy that the worst thing you can say about something is that there is no scientific evidence in support of it. There is no scientific evidence in support of anything one does. However, you believe in what you do, and you act as though it were true. Most of the time acting on that premise works out very well. You can circumscribe science any way you want to, and if you put those conditions on it, then I would have to agree that naturalistic inquiry is surely not science. If you say that the aim of science is to produce generalizations, and if you have a group of people who say they are not interested in that paradigm, then by definition, they cannot be scientists.

Question: Concerning the extrapolation of data, or interpretation of data, how would the rationalistic researcher go about interpreting the data versus the naturalistic researcher? The naturalistic researcher seems to conduct research without prior assumptions on a problem that is previously defined. The rationalistic researcher has reviewed the literature and defined a specific problem, or has set up a hypothesis and tries to disprove it. The example is the man who got on the horse and rode off in all directions. Which way did he go?

How does one know what is relevant and what is irrelevant? I think the answer to that is that I know in exactly the same way that scientists do. Scientists just look slicker because they started out with some questions or hypotheses, and that makes them look more structured. They also appear more rigid, by the way. Ask yourself where those hypotheses or questions come from. They came from some earlier consideration, which may, in fact, not have even been explicit. The particular questions or hypotheses to be investigated come out of the interaction between that problem and some kind of theoretical perspective that the scientist has chosen to bring to bear. This latter step is typically much more explicit because we require people to report it. We do not require people to report where they got the problem. We require them to report which particular theory they are going to use to handle the problem. They also are required to have some discussion about why they find that theory relevant to that problem. Now that makes it all look as if the scientist's mind is a great deal more structured and that obviously the scientist will know what is relevant to a much greater extent than someone using the naturalistic method. But naturalistic inquiry can start with precisely the same problems. Instead of formulating a priori hypotheses or a priori theories, naturalists cause them to emerge by looking at the phenomena studied. And how do I know what

phenomena to study? I do not study just any old thing. For example, if we brought twenty observers into this room and told them to describe today's events, without more direction than that, of course they would come up with completely different and unrelated descriptions. We might have another person come in who is an expert in large group instruction and who might observe what I am doing here and ask, "Did you ever hear a worse lecture in all of your life?" All sorts of descriptions might appear. But what if we sent twenty people in here with some problem to focus on? Let us imagine that the problem relates to large group instruction and its relative effectiveness. Then, I think they would make much closer observations. If someone dropped a bag full of garbage on the floor, none of the observers would think that was relevant to the problem. But if somebody raised a question from the floor that impinged on the problem, the observers would recognize that as relevant. So for me as a naturalist, those judgments are made continuously as I go along. They are refined over time as my grounded theory begins to sharpen up and as I begin to focus in on sensible questions to ask; questions that I could not have known in advance.

Question: What is the difference between the naturalistic approach and quasi-experimental designs? Campbell and Stanley's criteria for choosing a design were based on internal validity. Yours is based on external validity. I do not see a difference.

Campbell and Stanley do not move out of the scientific paradigm for a second. They are making a slight bow toward the realities of the world. Unfortunately, there are scalawags who prevent scientists from finding truth wherever they find it. They will not let us do such things as match groups or make random selections or assignments. So we do the best we can, and therefore, we have to fall back on quasi-designs. In principle, there is no difference in the terms of the assumptions on which that approach rests. What they are saying is that we have to make some concessions to the realities of life. Then Campbell and Stanley say to use multiple techniques because each of the techniques has a weak point in it somewhere. It is not like a true design. If you use the techniques in tandem and pick them wisely, the weak point in one design is covered by a very strong point of another design and eventually you can get them to work together. In my opinion, quasi-designs are approximations to true designs and they urge us to use them because the real world militates against our use of the true designs. They do not idffer in their intent at all. What you are trying to do, the nature of the inquiry, the kinds of problems you would raise, the methods that you would use, only allow you to make certain compromises. For example, instead of being able to make a group comparable ahead of time, by random selection or assignment, you try to find groups that are similar by getting some of their characteristics and seeing if they are similar in terms of means and standard deviations or whatever. The researcher does ex post facto matching rather than a priori. It seems to me to be an approximation or patching up of some inherent difficulties when one applies quasi-designs in the real world. Not for a minute does it mean a different view of reality. Researchers are still trying to produce generalizations. They still think there's only one reality out there and their inquiry will converge on it.

It is certainly true that the differentiation that Campbell and Stanley make between true designs and quasi-designs are not of both external and internal validity, but more of internal validity. There's a small bow in the direction of external validity. You may remember that Gene Glass took them to task by indicating that they had greatly underestimated the importance of their suggestions for the influence or impact that it would have on external validity. As a matter of fact, in a paper on trustworthiness, I took those eight threats that Campbell and Stanley talk about and said, "Suppose I were to apply these to qualitative methods within a naturalistic framework, what would I come up with?" My conclusion on that analysis was that there was only one of the eight in which the naturalistic paradigm might be likely to come out worse. It came out at least as well or better on the other seven. As for your assertion that you do not see any difference between the naturalistic approach and what Campbell and Stanley call a quasi-experimental approach, I do not know what to say about

except to rehash again that what they say is still predicated on a fundamental set of assumptions. They do not pretend that they are moving out of an inquiry paradigm. They are just talking about ways to patch up true experimental designs.

Question: Can I comment on your reference to sloppiness in research? It seems as if you are pointing out the necessity of having a professional with precise skills to carry out naturalistic research. Are you writing or planning to write something on criteria or guidelines on qualifications that should be required of naturalistic researchers?

Before I answer the question, I want to point out that it would be at least equally valid for me to raise a similar question with respect to the rationalistic paradigm. But no one ever seems compelled to do that. No one ever says, "Doesn't it take a special person with a special kind of training to use the computer and apply appropriate measurement methods?" Everybody takes it for granted that people will have been so trained. It is certainly true that it is possible to do sloppy research within any paradigm, if the person who does it is an incompetent person, not appropriately trained. I point that out because people tend to believe that lack of training is a unique problem to naturalistic approaches. I do not think it is unique at all. But in a more direct answer to your question, surely there are some skills that one would want such people to have: personal characteristics and modes of operating. In the book by Yvonna Lincoln and myself, *Effective Evaluation*, there is a chapter in which we begin a look at the topic of using the evaluator as, or the human as, an instrument. The chapter actually has three parts. One is a section that deals with the trade-offs that you gain by using a human as the instrument, a second one deals with the desirable characteristics people ought to have if they want to be instruments, and a third part deals with things you can do to improve the instrument, such as when you are working with a paper-and-pencil test. If, for example, you are not satisfied with its reliability, there are things that one can do to improve the reliability of the test. So there are things that one can do to improve humans as instruments. They are, by far, more infinitely perfectable than a paper-and-pencil test.

Question: If naturalistic inquiry is a new research paradigm, does the existence of new methodology suggest new kinds of problems?

I expect so. I do not think that that is uniquely a function of the naturalistic paradigm. I think that any time you put yourself into a different posture, you are going to see some problems that you had not seen before. We could have quite an interesting discussion about that. However, I do not think that it is particularly germane to the point I was making with respect to establishing bounds, which was simply to say, "How do you know what to include or exclude?" My answer to that was ruled by the nature of the problem. You will certainly find many places to apply it that you might not otherwise have thought of before. In a sense, that constitutes some new problems that will surely occur. I think that it will be healthy and I would be glad to see that happen.

Question: Are you concerned about use of results from a naturalistic study? If a study cannot be generalized easily, who is going to use it? As you commented earlier, the point is, we are trying to give taxpayers information on research. How can we best do that?

Those are certainly important questions. I hear those questions all the time, along with a lot of others that might relate to the general topic of the practicality of naturalistic inquiry. "We do not have time or the resources to provide information to taxpayers." "They will not understand the

results; they will not use the data; they will not underwrite the research in any way when so little can be specified from it." A whole series of problems of that sort exists. Yet, I do not know what to do about them. I am chagrined because students cite them to me as a kind of indication for doing something else. They say, "Since it is so inconvenient to use the naturalistic paradigm, why don't you let us do a survey questionnaire like all of our other friends in the department do who don't happen to have you on their committee?" My response to that is I am sorry if the naturalistic method causes extra work or extra concern for the student, but the practical contingencies that we can point to do not give one license to do bad research, especially when you know it is bad research. To say, "I can't do the job well, I don't have the time or money so instead I'll do it poorly" cannot be morally justified. So I am concerned about this issue, and while I am puzzled as to what to do about it, we must not back away from establishing the naturalistic paradigm's trustworthiness to taxpayers and policymakers.

Question: How can I locate your new paper that came out in summer 1981?

It is called the *Trustworthiness of Naturalistic Inquiries,* and it was published in the *Education Communication and Technology Journal.*

Question: I am bothered by your implication that we must use either one method of inquiry or the other—that there is no point of compromise. My background is in history and historians use the naturalistic paradigm, so I am familiar with it; but in vocational education, this area appears to benefit from the use of the quantitative paradigm. Is there some future time when we will be able to benefit from both the quantitative and qualitative methods?

I do not see a time when the two methods can be merged, but then, it's the spirit of your question that leads me to differentiate between what I am now calling fundamental axioms versus postures. Now I would regard the struggle of quantitative versus qualitative as one of the postures. There is a lot of literature now that would lead one to believe, if you did not know better, that the whole discussion is about quantitative versus qualitative methods. I do not believe that for a minute. I think it is a discussion differentiating between paradigms, not qualitative and quantitative research. So there is no reason in the world why you could not support naturalistic inquiries with both qualitative and quantitative methods. Similarly, you could support scientific inquiries with both qualitative and quantitative methods. It happens that there is a high correlation in practice in one direction, but there is nothing compelling about that, it does not have to be that way. Most of the postures, the preference of a priori versus grounded theory, for example, or the emphasis on rigor over relevance, are all matters on which I think compromise is not only possible, but desirable. I urge people to look for some kind of balance. But those postures are very different from these so-called fundamental axioms. I just do not see how you could possibly compromise between one reality and multiple realities; you must make a choice. I do not see how you can compromise in thinking that you have no interaction with a respondent or that you do. I do not see how you can compromise with saying generalization is possible or it is not possible, or that whatever statements you make are always context mediated versus context free. I just do not see how there can be compromises on those questions, any more than there can be a compromise between quantum physics and Newtonian physics. They are just different.

NOTES

1. In my earlier writing I referred to this paradigm as the "scientistic," but I was persuaded to alter this apparently pejorative label to "scientific" by well-meaning friends who pointed out that nothing was gained by projecting hostility. After some time other friends suggested that I was doing the naturalistic paradigm a disservice by labeling its competitor with such a highly respected name; since, in our culture, science is so highly valued that, by implication, its adversary must be less valued. I then became aware that the term "rationalistic" was coming into more common use to describe a complex of related ideas, such as the rationalization of administrative theory described in Perrow (1981). I have therefore adopted the same term here. Readers acquainted with my work should recognize that I intend the term to cover everything implied by my earlier use of "scientistic" and "scientific."

2. Ray C. Rist has reminded me that the German word for science is "wissenschaft," based on the root, *wissen,* to know. But German has two words for *know: wissen,* to have knowledge of, and *kennen,* to be acquainted with. One knows (wissen) scientific facts, but one knows (kennen) people. I am tempted to equate Polanyi's terms *propositional knowledge* with *wissen* and *tacit knowledge* with *kennen.* It is then but a short step to suggest that if the rationalistic paradigm is based on *wissenschaft,* then the naturalistic paradigm might be said to be based on *kennenschaft;* a neologism, to be sure, but a useful one.

3. I am indebted to Tom Schwandt, a graduate student in vocational education at Indiana University, for helping me understand the consequences of science's value-free assumption. For two recent examples that deal with this question, see Krathwohl (1980) and Guba and Lincoln (forthcoming).

4. The discussion to follow draws heavily upon Guba and Lincoln (1981).

5. Of course the rationalist cannot demonstrate internal validity by testing for isomorphism directly. To make such a test, one would need to know what reality was like; but if that were possible, there would be no need to study it. What the rationalist does in the face of this dilemma is to *disconfirm* competing hypotheses—the major hypothesis (the one the inquirer believes in) can never be directly confirmed.

6. I am aware of the fact that findings or interpretations may be declared incredible for illegitimate reasons; people sometimes do engage in self-deception or even lie when it is easier to declare an assertion false than it is to face up to its implications. Techniques exist for handling this problem, however. See for example Douglas (1976) and Guba and Lincoln (1981).

7. It is not entirely clear what such a journal should contain. I believe that at a minimum it should have four parts: (1) an initial statement of expectations by the investigator about what he or she thinks will be found; this statement can be checked against other versions written at later stages of the inquiry (say, at three-month intervals) to help decide whether the investigator continues to see the situation in terms of the original expectations or whether the original conceptions are altered over time, (2) a diary containing the investigator's day-to-day entries about anything considered important or meaningful, including the individual's own emotional reactions, (3) a set of methodological notes, in which the investigator documents each decision made that moves the investigation into next steps or that alters its direction in some way, and (4) a set of continuing data analyses based on all information collected to date that will be used to plan next steps and/or form the basis for the final analysis and interpretation.

REFERENCES

Bronfenbrenner, Urie. "The Experimental Ecology of Education." *Educational Researcher* 5 (October 1976): 5-15.

Douglas, Jack D. *Investigative Social Research.* Beverly Hills, CA: Sage Publications, 1976.

Glaser, Barney G., and Strauss, Anselm L. *The Discovery of Grounded Theory.* Chicago, IL: Aldine Publishing Company, 1967.

Guba, Egon G. *Toward a Methodology of Naturalistic Inquiry in Educational Evaluation.* Center for the Study of Evaluation Monograph Series, No. 8. Los Angeles, CA: CSE, UCLA, 1978.

Guba, Egon G. "Criteria for Assessing the Trustworthiness of Naturalistic Inquiries." *Education Communication and Technology Journal* 29 (1981): 75-92.

Guba, Egon G., and Lincoln, Yvonna S. *Effective Evaluation.* San Francisco, CA: Jossey-Bass Publishers, 1981.

Guba, Egon G., and Lincoln, Yvonna S. "The Place of Values in Needs Assessment." *Educational Evaluation and Policy Analysis.* forthcoming.

Krathwohl, David R. "The Myth of Value-Free Evaluation." *Educational Evaluation and Policy Analysis* 2 (1980): 37-45.

Kuhn, Thomas S. "The Structure of Scientific Revolutions." *International Encyclopedia of Unified Science.* 2d ed., Vol. 2, No. 2. Chicago, IL: University of Chicago Press, 1970.

Lincoln, Yvonna S., and Guba, Egon G. "The Distinction Between Merit and Worth in Evaluation." *Educational Evaluation and Policy Analysis* 2 (1980): 61-71.

Perrow, Charles. "Disintegrating Social Sciences." *New York University Education Quarterly,* Winter 1981, pp. 2-9.

Polanyi, Michael. *Personal Knowledge.* New York: Harper and Row, 1958.

Reinharz, Shulamit. *On Becoming a Social Scientist.* San Francisco, CA: Jossey-Bass Publishers, 1979.

Ruby, J. "Exposing Yourself: Reflexity, Anthropology, and Film." *Semiotica* 30 (1980): 153-79.

Scriven, Michael. "Objectivity and Subjectivity in Educational Research." *Philosophical Redirection of Educational Research.* 71st Yearbook of the National Society for the Study of Education, Part 1. Edited by Lawrence G. Thomas. Chicago, IL: University of Chicago Press, 1972, pp. 94-142.

Spradley, James P. *The Ethnographic Interview.* New York, NY: Holt, Rinehart, and Winston, 1979.

Webb, Eugene J.; Campbell, D. T.; Schwartz, R. D.; and Sechrest, L. *Unobtrusive Measures.* Chicago, IL: Rand McNally Publishing Company, 1966.

Zukav, Gary. *The Dancing Wu-Li Masters: An Overview of the New Physics.* New York: Bantam Books, 1979.

LEADERSHIP SERIES
IN VOCATIONAL AND CAREER EDUCATION

Apker, Wesley. ***Policy Issues in Interrelating Vocational Education and CETA***,1979 (OC 56—$1.90).

Baker, Eva L. ***New Directions in Evaluation Research: Implications for Vocational Education***,1979 (OC 55—$1.90).

Bienstock, Herbert. ***New and Emerging Occupations: Fact or Fancy***, 1981 (OC 77—$1.90).

Broudy, Harry S.***Toward a Theory of Vocational Education***, 1981 (OC 73--$1.90).

Clark, David L. ***Research and Development Productivity in Educational Organizations***, 1978 (OC 41—$2.20).

Cohen, Wilbur J. ***Needed Federal Policy in Education for Century III***, 1977 (OC 24—$1.90).

Day, Sherman. ***Education and Training in the Criminal Justice System: Implications for Vocational Education Research and Development***, 1979 (OC 52—$1.90).

Delacruz, Joseph B. ***Educational Programs for Native Americans: Implications for Vocational Education Research and Development***, 1978 (OC 40—$1.90).

Delker, Paul V. ***Adult Education—1980 and Beyond: Implications for Research and Development***, 1979 (OC 59—$1.90).

Dunham, Daniel B. ***Vocational Education: Policies, Issues, and Politics in the 1980s***, 1980 (OC 65—$2.20).

Ellis, John. ***Vocational Education and Federal Priorities***, 1978 (OC 47—$1.90).

Ellis, Mary L. ***Vocational Education: The Future Is Now***, 1978 (OC 37—$1.90).

Emmerij, Louis. ***National Strategies for Coping With Unemployment: An International Perspective***, 1981 (OC 69—$1.90).

Etzioni, Amitai. ***Reindustrialization and Vocational Education***, 1981 (OC 76—$1.75).

Evans, Rupert E. ***Vocational Education and Reindustrialization***, 1981 (OC 75—$1.90)

Feldman, Marvin. ***Work, Employment, and the New Economics***, 1981 (OC 70—$2.20).

Ganzglass, Evelyn. ***The Knowledge Development Plan of the Office of Youth Programs: Implications for Vocational Education Research and Development***, 1980 (OC 63—$2.20).

Gideonse, Hendrik. ***A Model for Educational Research and Development: 1985***, 1978 (OC 44—$2.20).

Glover, Robert W. ***Apprenticeship in the United States: Implications for Vocational Education Research and Development***, 1980 (OC 66—$1.90).

Guba, Egon G. ***The Paradigm Revolution in Inquiry: Implications for Vocational Research and Development***, 1981 (OC 79—$2.80).

Halperin, Samuel. ***Emerging Educational Policy Issues in the Federal City: A Report from Washington***, 1978 (OC 42—$2.20).

Hampson, Keith. ***The Relationship of School and Work: A British Perspective***, 1979 (OC 57—$1.90).

27

Herr, Edwin L. ***Work Focused Guidance for Youth in Transition: Some Implications for Vocational Education Research and Development***, 1978 (OC 43—$2.20).

Hicks, Laurabeth L. ***Programs of Guidance and Counseling Becoming of Age: Implications for Vocational Education R&D***, 1977 (OC 25—$1.75).

Jennings, John F. and Radcliffe, Charles W. ***Commentary on Legislation Affecting Vocational Education Research and Development***, 1977 (OC 27—$1.90).

Kolstoe, Oliver P. ***Implications of Research Findings on Vocational and Career Education for the Mentally Handicapped***, 1977 (OC 33—$1.90).

Kruger, Daniel H. ***Occupational Preparation Programs: Implications for Vocational Education***, 1977 (OC 31—$1.90).

Lecht, Leonard A. ***Vocational Education as a Participant in the Economic Development Enterprise: Policy Options for the Decade Ahead***, 1981 (OC 74—$2.20)

Levitan, Sar A. ***The Unemployment Numbers Is the Message***, 1977 (OC 38—$1.90).

Lund, Duane R. ***The Role of Vocational Education in the Economic Development of Rural Areas: Implications for Research and Development***, 1980 (OC 62—$2.20).

McCage, Ronald D. ***The Development of a Comprehensive State Capacity for Program Improvement***, 1978 (OC 34—$1.75).

McCune, Shirley D. ***The Organized Teaching Profession and R&D***, 1977 (OC 29—$1.90).

Martin, Edwin. ***New Directions in Vocational Education for the Handicapped: Implications for Research and Development***, 1978 (OC 35—$1.75).

Moody, Tom. ***Vocational Education, CETA, and Youth Unemployment: Meeting the Needs of Inner City Youth***, 1979 (OC 50—$1.75).

Musick, Craig D. ***Problems and Issues in Industry-Sponsored Vocational Programs: Implications for Research and Development***, 1980 (OC 67—$2.20).

Petty, Reginald. ***Trends and Issues in Vocational Education: Implications for Vocational Education Research and Development***, 1978 (OC 46—$1.90).

Pierce, William. ***Current and Emerging Structures for Education and Training: Implications for Vocational Education R&D***, 1980 (OC 68—$2.20).

Pucinski, Roman. ***The Role of State and Local Advisory Councils in Vocational Education***, 1978 (OC 36—$1.90).

Reider, Corinne H. ***Women, Work and Vocational Education***, 1977 (OC 26—$1.90).

Schergens, Becky L. ***The Parent's Role in Career Development: Implications for Vocational Education Research and Development***, 1980 (OC 60—$1.90).

Schmidt, Hermann. ***Current Problems of Vocational Education in the Federal Republic of Germany***, 1979 (OC 54—$1.90).

Shannon, Thomas A. ***The Role of Local School Boards in the Development and Direction of Programs of Occupational Education***, 1980 (OC 58—$1.90).

Sticht, Thomas G. ***Literacy and Vocational Competence***, 1978 (OC 39—$2.80).

Striner, Herbert E. ***The Reindustrialization of the United States: Implications for Vocational Education Research and Development***, 1981 (OC 71—$2.20).

Sullivan, Dennis J. ***Improving Productivity in the Work Force: Implications for Research and Development in Vocational Education***, 1981 (OC 72—$2.35).

Taylor, Daniel B. ***Revitalizing the American Economy: A Research and Development Focus for the 80s***, 1980 (OC 64—$1.90).

Tolbert, Jack F. ***The Role of Private Trade and Technical Schools in a Comprehensive Human Development System: Implications for Research and Development***, 1979 (OC 53—$1.90).

Wallace, Bertran F. ***Desegregation and Its Implications for Vocational and Career Education***, 1977 (OC 30—$1.75).

Wills, Joan. ***Youth Unemployment: Implications for Vocational Education R&D***, 1977 (OC 32—$1.75).

Wirth, Arthur G. ***Alternative Philosophies of Work: Implications for Vocational Educational Research and Development***, 1981 (OC 78—$1.90)

Wirtz, Willard R. and Ford, Gerald R. ***Bringing the World of Work and the Institutions of Education Closer Together***, 1977 (OC 28—$1.75).

ORDERING INFORMATION

All prices include postage and handling. When ordering use series numbers and titles. Orders of $10.00 or less will be accepted on a cash, check, or money order basis only. Purchase orders will be accepted for orders in excess of $10.00. Please make check or money order payable to: **The National Center for Research in Vocational Education**. Mail remittance and/or purchase order to: National Center Publications, The Ohio State University, 1960 Kenny Road, Columbus, OH 43210. (Prices subject to change.)

The Lecture Series at the National Center for Research in Vocational Education was established to provide a forum for discussing current issues confronting educational research and development among distinguished professionals and National Center and Ohio State University staff. Points of view or opinions do not necessarily represent official National Center or Ohio State University position or policy.